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Economics as a Science

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What is a Science?

The plan is to discuss what is a science and whether economics fits the bill and some hidden sources of power. My predecessor but several before at the London School of Economics, Karl Popper, thought he had the question of ‘What’s a science?’ solved. Scientific claims, you all know this, he maintained, are falsifiable. I am now quoting from Popper: ‘I’ve found that those of my friends who were admirers of Marx, Freud and Adler were impressed by their parent explanatory power. These theories appeared able to explain practically everything that happened within the fields to which they referred. It was this fact that they always fitted, that they would always confirm, which in the eyes of their admirers constituted the strongest argument in favour of those theories. It began to dawn on me that this apparent strength was in fact their weakness.’

Here is an example that would have been dear to Popper’s heart and it is just the kind he gives himself, the Rat Man, according to Freud, had an unconscious desire to hurt his father and this could, of course, result in him quite unexpectedly being nice to father, as we know through various Freudian mechanisms, or equally being nasty to his father. So the hypothesis was consistent with quite incompatible bits of data.

Now, the trouble is if we adopt Popper’s criterion it lets in too much. So the claim that I am not sitting any longer at my desk in Durham or in UCSD at this very moment is falsifiable, but it is certainly not science. You need to add a whole lot more and it is the ‘whole lot more’ that does a lot of the job. The problem is that we have had a lot of trouble figuring what the whole lot more to add is. The other is that it rules out too much. Physics has exactly the same problem. The very same hypothesis about a situation can imply very different observations. A nice physics case: ionised thallium undergoes beta decay is the hypothesis and it implies two observations which are incompatible with each other and the reason is, of course, the physics solution to this is the obvious one we all know, that which observations are implied depends on what other empirical facts are taken to obtain in the situation. But that’s Freud’s solution too. That was exactly Freud’s solution and, of course, one can then begin to puzzle out whether you can then put some constraints on these other auxiliary assumptions.

The long and short of it is that after 60 years of work in my field in the philosophy of science, very serious work on this issue, and not only in the philosophy of science but elsewhere. People are very concerned about whether climate change deniers, when they produce papers, are really producing science; people in the US concerned about whether you can teach creationism in the public schools as a science along with evolution; the US Supreme Court’s *Daubert* ruling on what can count as scientific expertise; none of these have come up with any satisfactory criterion. There is no good criterion to demarcate science from non-science. This past autumn, the International Philosophy of Science Association, which meets every two years, after many, many years of the issue lying dormant, had a session on the scientific method – what is the scientific method? And four people, really top of the field, really thinking hard about it, and nothing serious came out of that.

Robert Skidelsky

We should feel more cheerful about economics.

Nancy Cartwright

So it is a little hard to answer the question ‘is economics a science?’ Not a clue.

Is Economics a Science?

The thought I was supposed to address was whether economics has peculiar power as a science rather than, for instance, as an adviser to public policy, running the Bank of England etc. The first thing, I think, to note is that the power stemming from economics as science truly depends not entirely on the truth of the claim that it is a science but on the perception and there is wide perception that economics is a science and, as it was mentioned earlier, a particularly good science because it is objective. So it cannot be denied that economics gets special kudos in policy areas and in the public because it is thought to be, it purports to be, it is widely believed to be objective and part of the reason for that is that it is that it is quantitative. Quantitative is thought to be particularly objective. I will not go into this much – you are probably familiar with Ted Porter’s *Trust in Numbers* and Michael Power’s *The Audit Society*, both of which describe both the history and some sociology of how we have all been converted to the idea that if something is quantitative it is objective and if it is not quantitative it is not objective.

Hidden Sources of Power

I would like to go on to talk a bit about hidden sources of power. Economics as a science, because it produces knowledge and knowledge claims, has sources of power, and sources of power that many of you might recognise but certainly are not publically recognised. The two I wanted to talk about are looping effects and then the hidden power that comes through the design of measures and walls.

Looping effects have a number of other names: performativity, looping effects, reflexivity – that’s the word that George Soros likes so much – self-fulfilling prophesies. I will just give you one case of that by Donald MacKenzie, the sociologist at Edinburgh, who has done this nice study. Again, I would hope this is something you are surely familiar with. So this is MacKenzie: ‘Option pricing theory succeeded empirically not because it discovered pre-existing price patterns, but because markets changed in ways that made its assumptions more accurate and because the theory was used in arbitrage. Option pricing theory did not simply describe a pre-existing world, but helped create a world of which the theory was a truer reflection. By the second half of the 1970s discrepancies between patterns of option pricing in Chicago and the Black-Scholes model diminished to the point of economic insignificance. The reasons include the use of the Black-Scholes model as a guide to arbitrage. Black set up a service,’ – I always love this; I didn’t actually know this before. You probably all knew it but I did not know it before I read MacKenzie – ‘Selling sheets of theoretical option prices to market participants. Option market-makers used those sheets and other material exemplifications of the Black-Scholes model to identify relatively over-priced and under-priced options on the same stock, sold the former and hedged their risk by buying the latter. So,’ MacKenzie concludes, ‘in so doing they altered patterns of pricing in a way that increased the validity of the model’s predictions.’

So that is one source of power that you have these looping effects. There is a very explicit case where there is a clear causal chain that MacKenzie traces. The other, of course, is Michel Foucault’s theme that anyone who was able to create a new category or new concept that comes to be prominent and dominant is a hidden source of power because what happens

is as this concept becomes dominant people begin to use it and people self-identify who are in the category and begin to identify others as in the category and treat you in the ways described. Like the involuntarily unemployed, the old one of the 'deserving poor' and so forth. That is one source of power that is not always so obvious to people outside economics.

Another is the hidden sources of power in designing measures and models. I am going to talk about measures first and I am just going to take cases from Tony Atkinson. I might mention to you that the hidden sources of power in designing measures and designing models are sources of power over people's lives, but they are places where having economic knowledge really matters and you would not know what you were doing if you did not have this economic knowledge. I teach this material when we talk about whether or not economics can be value-free; whether it is objective in the sense of being value-free. These are all places where making certain decisions, based reliably on knowledge that you have as an economist, will fairly predictably harm some groups of people and benefit others. There is no scientific reason to make the decision one way rather than another. Now you can, consciously or not, use this knowledge to settle for one group or another but it is a place where there is an easy place for the intrusion of values and certainly because of special economic knowledge that most people do not have and cannot see what difference it makes whether you do it one way or another, gives power over the lives of the people affected, given the natural uses that we know when we made these measures.

There are issues from Atkinson when you are thinking about designing a poverty measure. People can get the idea of the difference between an absolute and a relative measure; you can sometimes even get people to think about whether they want, if it is a relative measure, like two-thirds of the median income. But if you start asking about whether it should be the mean, the median or the mode you have lost people. Then he raises chapter after chapter of places where it makes a big difference to the poverty numbers and poverty ranking of different states and nations depending on how you design the measure in the detail – whether you choose relative versus absolute, mean versus median, whether you measure expenditure versus income, whether you treat households versus families, whether you use equivalent scales, numbers versus gaps. We know that Atkinson has been in favour of measuring a poverty gap, which is how deeply below the poverty line individuals are as opposed to the numbers, where it is a good strategy if you want to get your poverty numbers better to take the people at the top and push them over. But that is an issue of having a poverty number or a poverty gap measure.

Here is one really easy example from India. The Indian Statistical Institute asked people how much rice they consumed over the past 30 days. That is how they were measuring part of their poverty measure. Other countries tended to use a seven-day period. In response to criticisms that 30 days is too long a period and people do not remember how much rice they have consumed over the last 30 days, India changed their time period to seven days. And what happened? The technical change cut the Indian national poverty rate by half. By redesigning the measure, 175 million Indians suddenly escaped poverty. Those are the kind of issues that come up in the design of measures. Atkinson also does a study helping to design the EU measures for social exclusion. There is a huge variety of similar issues.

Another source of hidden power is in modelling assumptions, for instance, first of all, the choice of the model type. Here is another case I have taken from Tony. He draws our attention to the fact that the commonly used representative agent models conceal issues of distribution. This is a bit like Norbert's point where Norbert argued that the economic theory changed as the interests of the well-off as they changed. How it changed, in almost all of his cases, was it changed by burying issues. Certain issues were no longer able to be expressed; they were not expressible in the model. They become hidden. It is not that you cannot talk about them, but

you cannot talk about them when you are doing this proper economics within the model. Or, for instance, most models assume the aim is to maximise expected utility. Of course you can make utility the most abstract notion possible, but still there is a difference between measuring a course of action that maximises expected utility and one that maximises something like Amartya Sen's substance of freedom.

On modelling assumptions, choice of parameters is a very famous case. Nick Stern got in a great deal of trouble about his choice of parameters in the economics and climate change. The Stern Review begins with a maximise-expected-utilities model. Interestingly it is a representative agent model, so there is one representative from each generation and he himself admits that the Review does not really take on the distribution of responsibilities and benefits and gains for a generation, so he does not really talk about who pays rich countries, poor countries. That is concealed in the representative agent model, but that is upfront that he is doing that.

The question is how much weight we assign to each generation. If you look at the sum, you have a weight for each representative agent of each generation. You are all used to putting discount factors for the future, but you have to think about what this discount factor for the future means in this equation. There is a variety of reasons for discounting the future. For instance, they might not be there so you might want to count future generations a little less. It might be a very poor way of putting uncertainty into the model, that this is not where it belongs. When you put a weight in that model you are weighting how much utility that generation matters in the proposed policy. The discount factors really matter here and what is interesting to me is that once you have chosen an expected-utilities framework you cannot avoid this question. You can not write the W down there and then that means you are weighting everybody equally or you can discount some generations relative to others, but simply by virtue of using the expected-utilities framework you are forcing some, in this case, ethical decisions to be made.

The reason I was bringing it up in this context is not about the ethics of it so much as that if you look at the Stern Review you have to be fairly sophisticated to see what is going on there. It is using an expected-utilities framework and there is this issue about how future generations are treated, and a lot of debate about the exact form of the discount factors.

Concluding Comment

Just to review: the promulgation of economic claims, I have reminded you, can change the world. Even to making the world adjust to fit its false models. Moreover, details matter in measures and models. They affect policy and who benefits and who loses but the point is that it takes real economic knowledge to understand how in both those kinds of cases. My conclusion is: does economics have power because it is a science, because of those special kinds of knowledge that economics has? The answer is yes.

Discussion

Economics as a Science: Discussion

John B. Davis

Marquette University and University of Amsterdam

I will begin by identifying myself a bit. I was trained originally in analytic philosophy, not at Oxford but in the Oxford style. Then I was trained in economics, primarily history of economics. I am co-editor of the *Journal of Economic Methodology* and I chaired and taught in a History and Philosophy of Economics program for 10 years at University of Amsterdam, where the program focus was the History of Economics from 1980 to the Present. I was and am still especially interested in evolution of mainstream economics. A principal argument that I have made is that all the main new movements in mainstream economics are sourced from outside economics – behavioural economics, for example, from psychology. I was interested in what this meant for the state of economics. Sometimes I am charged with arguing economics exhibits ‘mainstream pluralism’. I will talk here about mainstream economics at this stage of its development as essentially a performative science. I want to emphasise the relation of economics to inequality and social stratification.

I think it is fair to say that we live in a world that is becoming increasingly unequal. It is also being institutionalised as such, and this works through structures that enhance and reinforce social stratification. I have worked with recent economics stratification theory as a foundation for self-reinforcing inequality and stratification processes that result from structures that systematically privilege higher and de-privilege lower socio-economic strata. Where is the science of economics in all this? The economics profession’s own stratification processes involve replacement of its traditional independent reflexive practices for the evaluation and assessment of economics research with a stratification-reinforcing journal-ranking system that perpetuates status quo economics, limits innovation in economics, and thus serves social stratification.

The effect of this process in economics, I suggest, is that scientific behaviour in mainstream economics is increasingly replaced by bureaucratic behaviour and economics increasingly functions as what I will describe as a performative science in the sense of a science that always sees the world in its own image. I suggest that mainstream economics then risks becoming a ‘bubble-science’, one that is vulnerable to collapse like alchemy and other failed sciences of the past, and as such a potential contributor to economic crises. Let me explain this in terms of the change in reflexive practice in economics.

What was previously the traditional form of reflexive practice in economics? In the past, (i) economic methodology and (ii) the history and philosophy of economics were economics’ reflexive domains; in effect its principal forms of scientific self-consciousness. Like other sciences, economics relies on a theory-evidence relationship. (i) Economic methodology explains the theory-evidence relationship as a reflexive relationship. Theory depends on evidence and what counts as evidence is influenced by theory. Yet because the economy itself evolves, there must always be new evidence, so for economic methodology theory is always evolving and there must always be new theory. (ii) The history and philosophy of economics then explains economics’ status as a science relative to the adequacy of its methodological practice, and in particular according to its ability to evolve as a science.

What is economics' new reflexive practice? Methodology and the history and philosophy of economics are now largely marginalised in the economics profession. Whereas those reflexive domains were the means by which research quality and economics' performance as a science was ultimately judged, research quality is now judged largely through journal-ranking systems. Comments have been made in the discussion here about the importance of institutions and apparatuses like the Research Assessment Exercise in the UK in sustaining journal-ranking systems. These institutions and apparatuses are status-quo-biased and reinforce social and theoretical stratification in the profession. Together, they reflect the famous Matthew effect: the rich get richer and the poor get poorer (from St Matthew), as described by sociologist Robert Merton.

In the overall dynamic, research from top institutions only goes to top journals, top journals only publish research from top institutions, and so top journals remain top journals and top institutions remain top institutions. I think that is now the main reflexive structure in economics. It has come about because in the last 25-30 years journal-ranking system has been put forcefully into place for judging how people are promoted, how their research is evaluated, and basically how the profession works.

Looking over this time period from the perspective of economic methodology and the history and philosophy of economics, the main development was the elimination of the history (and philosophy) of economics from most economics departments. At the same time, the main generalist journals in economics ceased to publish history and philosophy of economics research, so that most economists ceased to be exposed to it and increasingly regarded it as irrelevant to the practice of economics. That meant that the way in which economics practises or operates the theory-evidence relationship is no longer an issue of concern in the economics profession. Where does that then leave economic methodology in the economics profession? The history and philosophy of economics judge the adequacy of the profession's economic methodology. Minus those fields' influence, most economists now confuse economic methodology and economic method. The former is the epistemology of economics; the latter concerns the tools of economics, especially econometric method, mathematical modelling, and increasingly experimental method. When method replaces methodology, these tools cease to be evaluated in regard to how well they contribute to knowledge. This means evidence is more and more taken at face value since there is little reflection on what counts as evidence. I suggest the consequence of this development is that economics is becoming a performative science.

A performative science is one that actively seeks to remake the world – I emphasise 'seeks' because it cannot ultimately be successful – in its own image through policy and institutional design changes that incentivise behaviour to fit the theory. The MacKenzie research that has been discussed here is quite good on performativity in connection with the efficient markets hypothesis. Nudge behavioural economics is another example. Its policy recommendation is to alter social structures that incentivize people to behave as rational agents. Mechanism design theory may be even more important, because it aims to design entire market systems in such a way that people must behave as rational choice theory requires in order to be successful. What these initiatives thus do is seek to make the world, or 'perform' it, as standard theory sees it. I see the development of these approaches in mainstream economics as a natural outcome of the marginalisation of economic methodology (and the collapse of methodology into method). Without reflection on the epistemology of economics, economists become insensitive to the nature of the theory-evidence relationship and their role in determining it. Then they are vulnerable to seeing the world in the image of their own research.

How does this all fit together with the recent emergence of journal ranking systems as the main means of evaluating research in economics? If you do mainstream research, it is readily

identified as such, and so it possesses a self-validating character. In reflexivity terms, mainstream research then functions like a self-fulfilling prophecy. If you do mainstream research, since journal rankings identify this as good research, your research fulfils the requirement of being good research. The opposite is case with heterodox or non-standard economics. It is a self-defeating prophecy. By being identified as such according to the journal ranking system, it must go to non-top journals. Since non-top journals only publish lesser quality research, heterodox or non-standard research must be lesser quality research.

So we have as one of the outcomes of mainstream economics evolution as a performative science that it differentiates research practices according to where they originate in a stratified profession. This means many substantive topics are off the table for the mainstream of the profession, not only non-standard research, but such matters as the role of normative values in economics. Another way to put this is to say that economics is becoming an increasingly self-referential science.

I ask, then, is mainstream economics at risk of becoming a bubble science? A science that systematically rebuilds the world and its scientific practice in its own image is one that is likely to fail to explain a changing world. The failure of economics to anticipate and after the fact explain the financial crisis fits this picture. A bubble science, then, is one that will suffer significant stranded theoretical asset write-downs. We know from the history of science that this has occurred regularly. There have been many bubble sciences. Marxist economics was mentioned. Is neoclassical economics, its cold war compatriot that played a comparable ideological role, sitting at the end of a similar historical evolution?

It is interesting that mainstream economics seems to have become increasingly performative in a period when other sciences have gained greater influence within economics. I have written fairly extensively about the new movements deriving from other sciences in economics: complexity theory, behavioural economics, experimental strategies, neuroeconomics. They have all originated from outside of neoclassical economics. Thus they bring in deep reasoning from other sciences, 'contaminants' by the standards of neoclassical theory, and so we now have an economics ecosystem that is more diffuse and unclear in its overall character. I ask: is there a new reflexivity operating internal to economics generating new methodological and epistemological issues which runs counter to the mainstream's performative ambitions? Might this possible development again require a history and philosophy of economics able to judge economics' recent trajectory relative to its past development? A history and philosophy of economics that takes the present as history?

As a closing remark, let me comment briefly on how mainstream economics might adjust to these other-science influences. One thing that might happen is that key components of standard thinking get replaced piecemeal by new theory components that reflect other-science influences yet still comport with the main thrust of mainstream economics. I take as my example the theory of labour compensation. The standard view is that labour is paid its marginal product. Going back to the 1980s when game theory and behavioural economics began to influence economics, the Chicago School developed an alternative view of labour compensation called tournament theory. You are no longer rewarded according to your marginal contribution, but according to your success in a lottery among many equally qualified people. Successful individuals then gain employment and income, and are set apart in terms of rank and position appropriate to a stratified world. Lazear and others have shown how labour markets are efficient under this system. So the old neoclassical marginal reward analysis is put aside, but a mainstream competitive, efficiency-based account is preserved.

Interestingly, an economics that evolved in this way would be less bubble-like because it captures the real world phenomena of social stratification. It does so on the view suggested here because it accommodates other-science influences, albeit within its own traditional framework of competition and efficiency. I leave further reflection on this case to other occasions. What seems fair to conclude here, however, is that this kind of evolution of economics works quite well in a world in which a bureaucratic journal-ranking system explains how the science of economics operates. Thank you.

Robert Skidelsky

Thank you. You were both very thought-provoking. One thing I got from out of both was the question, obviously, to what extent does the word ‘science’ apply to any of these things? One of the things Nancy suggested was that the problems of validating economics as a science also apply to physics. I had always taken there to be a distinction.

Nancy Cartwright

I think there is a distinction; it is just we cannot come up with criteria that will neatly separate them and stand up to good reason.

Robert Skidelsky

Can we try and do that? Maybe someone will be able to. The other thing is the idea of the self-fulfilling prophecy, which is very much, in a way, a rational expectations view. The results of policy are model-dependent and if the model is bad then you cannot do good policy, even though it would have good results, because people do not believe in the model by which those good results would ensue. It is an appalling thought in a way that your bad models would prevent good policies. So these are the ideas that went through my head as I heard this but let us open it up.

Roger Backhouse

I could not help wondering whether Nancy’s examples, which I found very persuasive, are not really counterexamples to the extreme cases that John is building up of economics being totally reflexive. The examples she quoted of Atkinson and Stern are surely examples of something good that good scientific economics has done, despite all the pressures that John talked about.

One thing that struck me in Nancy’s point is the example of Stern. Once you formulate it in this way, you are forced to face up to certain issues, such as how you value one generation or another. Is this something that we say is characteristic of a science, in that the abstraction and precision that can be abused to construct rules that are only self-verifying and so on, nonetheless can force you to ask questions? If you were adopting a less formal method or using other methods, you would not be led to ask them. Maybe an economist is forced to ask difficult questions that turn out to be important, which Stern and Atkinson have asked, which other social scientists – I will not mention them by name – might not have asked.

Robert Skidelsky

Could you take that up immediately, because it seems a very important question?

Nancy Cartwright

I thought that too and agree. With respect to the detailed examples I was giving, it is important to notice that Atkinson is very old – well, my age – and so is Stern. They are part of a group of welfare economists, which includes people like Angus Deaton and Amartya Sen. Welfare

economics is not taught at the London School of Economics. I did not choose them just because people happen to know them. It is our business to look through the literature. I wonder what happens when these people die.

John Davis

In response to Roger, I did not mean to portray economics as monolithic; it is the rational choice part so central to the mainstream of the profession that I am targeting. If you look at Anthony Atkinson, Thomas Piketty, and others who are working on inequality, their work exhibits economics' traditional reflexive practices. Similarly, Sen has made important philosophical arguments about capabilities, and the Multidimensional Poverty Index developed by his followers at Oxford constitutes a contribution to the understanding of poverty measure and an advance on the standard headcount-based only or an income-alone measure. I believe there are serious scientific efforts to describe and explain real-world problems in much of economics. I am targeting rational choice as the performative mainstream.

Anthony Heath

I agree with most of what was being said, but there are a few points missing from the argument. The one I want to emphasise is the difficulty of testing an awful lot of economics empirically. That is one key point that explains why economics can get away with it, in a way that medicine cannot. You would not do the sorts of things economists do if you were making decisions in medicine. You would not use instrumental variable methods, for example, when you were deciding on treatment outcomes. Maybe economists would like their treatments to be decided on the basis of an instrumental variable model, but I suspect not.

Of course, instrumental variable models are used because it is extremely difficult to test and do experimental analyses. That is why the two examples you gave, which are very interesting ones, developmental behaviour economics and experimental economics, have both come in very particular areas of economics, where you can actually do proper rigorous testing. Behaviour economics came from Kahneman and Tversky, and has been around for 50 years. We have all known for 50 years that the behavioural assumptions in difference curves and consumer theory were unsound. The interesting thing is that it took 50 years for behavioural economics to really be developed, but it happened in an area where it was possible to do the empirical investigation. Experimental analysis takes place on a narrow range of topics where you can do the empirical investigation. The crucial reason why a lot of the influential economics is not a science is that it is too difficult to test. For most of the assumptions and predictions in economic theory, there is so little data and so many possible theories that there is a mismatch. That is the fundamental issue with interest in macroeconomics.

The other point Nancy made is that economics is quantitative. That is only half of the story. There is the quantitative work that I do, which is basically collecting data and testing hypotheses, with statistical techniques. There is the quantitative side of developing a model from axioms, which are assumed, which is a different kind of intellectual exercise. That is what fills the gap left by the inability to test most predictions of economic theory in any way that a scientist would recognise.

Norbert Häring

In response to the argument that this rigorousness and formalism can force you to ask questions, what economists are doing is to ask the questions of the mainstream in their way. Take the example of willingness to pay, which is a prevalent concept used by economists. It goes into cost/benefit analysis. It says that every dollar that anybody wants to spend is the same; it does

not matter how many dollars they have. That even goes to the cost/benefit analysis of the value of a statistical life. We as economists produce the result that a poor person, a black person or a woman is not worth as much. They call it statistical, but that is just an excuse to talk like that.

They reach a result that, if you do something that will save the lives of black people, it is not worth as much if you save the lives of white people. It is because of a concept they use, which they call willingness to spend and not ability to spend. With that concept, they bury the whole thing about how much money they have. I am not exaggerating; they are really saying that. They are saying that the statistical life of a woman is worth less than the statistical life of a man. It could be empirically tested; it is just prohibited.

Irving Fisher, in the 1920s, researched about how much money or income is worth to different classes of people statistically and what people do to research. It is obvious if you are not formal; a poor person bends down to pick up a penny and a rich person does not. That is clear. From what Lionel Robbins did, it was prohibited to do that kind of empirical research and it is not done anymore. You cannot publish anything by doing the kind of research that Irving Fisher did, which goes into the declining utility of income.

Robert Skidelsky

You are saying that the barriers to empirical research are less strong than Anthony indicated.

Norbert Häring

You cannot do that sort of empirical research.

Participant

Can I respond to Anthony's question and also Robert's question? I do not think that economics is unique in having to describe a very complicated world. If you take the example of astronomy or physics, before the Copernican revolution, people generally believe that the Earth was the centre of the universe and the sun went around the Earth. This of course did not cohere with many observations of how the planets actually moved but, because the universe was a big place full of many things, Ptolemaic astronomers could simply say that there were more asteroids out there, other planets or suns that were confusing the cycles of the sun and leading to what they called epicycles and extra little cycles, which then approximated to the movement we actually observed.

Those were described by Thomas Kuhn, a philosopher of science, as ad hoc embellishments of a failing paradigm. They saw that there was empirical evidence that could refute the theoretical core of that paradigm but, instead of saying, 'Okay, well let's move on to a different theoretical core,' they instead say, 'How can we adjust small things at the fringes of our theories to accept the evidence?' In some ways, behavioural economics does that for neoclassical economics.

The problem is not that that is a bad thing. Of course, you should not abandon your core beliefs at any instance of any person in the world giving you a counterexample. The problem is that, in economics – and this responds to Robert's question of how we can solve the demarcation problem – anybody who is engaged in a theoretical construction has not read any philosophy of science that was published since the 1950s. There is no recognition of there being a whole debate surrounding Imre Lakatos or Thomas Kuhn, which fixes the property of being scientific not to specific theories or observations, but to the nature and sociology of the whole paradigm and whether that paradigm embraces change or not.

If you ask most economists what makes their discipline scientific, they will either have nothing to say or they will refer back to Friedman's article on the 'Methodology of Positive Economics', which of course is not taking part in a broadly situated debate within the philosophy. It has no space for a deeper sociology of economics. Solving the problem of demarcation with a philosophy of science might be impossible, but it will certainly be impossible in economics if the debate is 50 years behind the debate in philosophy.

Adair Turner

I wanted to comment on this issue of empirically testable propositions, because what they produce are very different results in different areas of economics and, in particular, between some categories of microeconomics, macro and finance. The perspective in which I come to that is from doing a series of public policy jobs: as Chairman of the UK's Low Pay Commission, which sets the minimum wage; as Chairman of our Pensions Commission, which proposes changes to our private pension system; and as Chairman of the Financial Services Authority dealing with the financial crisis.

On the work of the Low Pay Commission, I did not feel that there was a catastrophic failure of economics to say useful things. Indeed, we found that much of the work on labour markets and the role of monopsony in labour markets, and some of the empirical work in the labour markets, helped give us a confidence that you could actually increase the minimum wage in a non-trivial fashion without producing the adverse job losses that a simplistic neoclassical model would suggest. That is available within the economic literature and it builds upon empirical observations.

When we did the Pensions Commission work, we drew very significantly on behavioural economics, on the experiments of behavioural economics and, in particular, the role of inertia. We ended up proposing a public policy development in the UK where you are now automatically enrolled into a funded pension scheme and you have to opt out, not opt in. We did that on the basis of a reading of the literature of inertia and nudge. The experiments, which people like Kahneman and others have been doing for years, about the way that decisions are driven by the way that they are presented to people, have been incredibly successful. The extent to which people have stayed in, rather than opted out, has exceeded our expectations and is now driving an increase in useful pension savings.

When I got to dealing with the financial crisis in 2008, I found pretty much the whole of modern macroeconomics no help whatsoever. Most of modern finance theory, which I shall talk about later, is positively misleading in its explanations of what occurred. What is partly going on here is that, when you are dealing with the issue of what will happen when you increase a wage rate, you can observe some experiments that have occurred in City X or City Y and what happened. With the behavioural stuff, there is a whole body either of experiments or things that give you the same effect, real-world experiments. Some of US 401(k) plans use auto-enrolment, opt out not opt in, so we were able to build on the empirical stuff.

The difficulty with macro is that you cannot run Economy A for the next 10 years in one fashion, Economy B in another fashion and then observe which one performs better. The size at which you are operating makes controlled experiments impossible. That is inherent within some of the most important issues of economics, the macro-issues, which are not subject to an empirical test. You cannot run an empirical test.

What that means is that economics must not migrate solely to try to answer the things that it can answer. For instance, in some areas of development economics, there has been a collapse to incredibly micro, somewhat interesting but really second-order, issues, which can be subject to randomised control tests in the field, and migration away from some of the bigger issues.

Why is Korea now twenty times the GDP per capita of Congo, whereas it was very slightly behind in 1950? We have to be aware that the different degrees by which we can test different propositions can, unless we are careful, drag us towards saying things on micro issues and avoiding some of the most important issues in which economics has to engage. .

Jamie Galbraith

As I listen to your running through these categories, in each case, an example came to my mind that was sort of like the one you were presenting, but not quite. I would call it a pseudo-example of what you were saying. The first category was this question of the power of quantitative reasoning, what is called quantitativity. Here, two pseudo-cases came to my mind, both drawn from the same source, namely the recent book by Thomas Piketty, one of which is his elaborate and very persistent trumpeting of the value of the data work that he has done, which is, whatever its value may be, drawn from income tax records from a selection of countries, which is by itself a very limiting group of countries. You have to draw on countries that actually have income tax. That would normally require you to be very careful about the different definitions of income and changes in the tax model in order to ensure that you have comparable measures. It is not at all clear, when you examine that material closely, that that level of care had been taken before comparisons were made.

The other pseudo-example that comes from the same source is the notion of the concept of capital as an entity. The literature in economics establishes pretty persuasively, although most people have forgotten that it was ever written, that the concept of capital is not quantitative. That is entirely swept aside in this argument. I would suggest that what one has here is not the power of quantitativity, but the power of pseudo-quantitativity in these cases.

The second area raised was looping effects, where the theory influences the fact. A nice instance of a pseudo-looping effect is exactly an extension of the one you mentioned, with respect to the black schools model. The ultimate implementation of that was in the design of the trading strategies of long-term capital management, which blew up most spectacularly in 1998, as the entire world diverged from the predictions of the model, because of a flaw, a basic fallacy, in the distributional assumptions, the shape of the tail, of events in financial markets. There is a question of a pseudo-looping effect, with a nice eventual bite back from the real world.

The third area of category you talked about is the power of concepts. Let's call it concept power. What came to my mind was at least one instance of pseudo-concept power. The phrase that comes to mind here is the phrase New Keynesianism, which is a case of semantic grave-robbing. It is intended not to develop and advance an understanding of the ideas of John Maynard Keynes, but in fact not only to replace those ideas with their contrary concepts compatible a neoclassical framework, but to make the ideas of Keynes essentially even more inaccessible, because you have to sort through what this word 'Keynes' actually means. It puts the actual person and his writing at an extra remove that makes it much more difficult to understand. I would suggest, to modify your closing line, is economics powerful because it is a science, to put the word 'pseudo' before 'science' and then you probably have a case.

A nice example of this, which comes to the point about pop journals and institutions, is an incident that occurred a couple of months ago in France, which has about 2,400 self-identified professional economists, of whom 600 identify themselves as heterodox. Those 600 set out to persuade the ministry that governs these things to establish a separate part, so they could be self-regulating and have a professional ladder that was not entirely blocked by the 1,800.

The response of the two leading figures in mainstream French economics, Mr Tirole and Mr Aghion, two of the very leading figures, was to write to the minister and say that all 600 of

the heterodox economists were inferior to all 1,800 of the mainstream economists, a proposition that is perfectly possible if you have a purely stratified system and consider them to be two separate categories, which would be difficult to accept. If you admit that, if you have them at all, you should allow them to judge themselves. It is completely impossible to accept if you admit that they are economists and subject to some distributional laws. Surely there must be 600 who are literally better than the worst of the 1,800. The fact that Mr Tirole and Mr Aghion could not grasp that point demonstrates, it seems to me, the difference between their view of rankings and the ordinary.

Participant

They should use a [inaudible] distribution then they could pull it off.

Jamie Galbraith

I do not think they were quite up to making that argument, because it would depart from the mainstream perspective and force them to move over to the domain of the dreaded econophysicist.

Robert Skidelsky

I suspect that they were perfectly capable of making it, but just did not want to. On that particular incident, Professor Tirole, who is a French Nobel prize winner, wrote to the minister and said, 'We cannot have two communities of economists. There has to be just one and it has to be mainstream.'

Jamie Galbraith

It is basically an exterminationist view.

Robert Skidelsky

'Send them off to other departments.'

Andrew Graham

I cannot cap that, but wanted to end by taking issue with Adair Turner on the micro/macro story. His version of it, put more subtly than this, was that what we are doing in microeconomics is helpful and can get us somewhere and macroeconomics is much more difficult. Let me go sideways.

We started by asking whether economics is a science, and Nancy gave us a very eloquent exposition about why how even the best brains in the world cannot assemble any criteria that would allow us to distinguish physics from economics, in one way or another. I am willing to live with that, but would like to return to the book by Bernard Williams, *Truth and Truthfulness*. He thinks we can almost always tell whether people are trying to be truth tellers. He does not make the silly mistake of thinking there is such a thing as truth, but he thinks we can tell whether people are trying to do it or not. You can apply that to good historians, economists or physicists. Almost always you would do it by looking at more than one truth-telling story. There would be things they would share. Point one in this bit is that we are radically missing, in economics, alternative stories that students are then asked to look at to say who is the truth teller or what they could boil out of that that they would like to hold on to.

If I was a truth teller who then looked at microeconomics, the assumption that the representative agent is always, everywhere, rational in the economic sense is just obviously not true. It is

blindingly obvious that they are trying to pull the wool over our eyes. Look at obligations, commitments and all the things about which Adair spoke, such as inertia, if you are in one position, you would be reluctant to move to the other. The whole thing wrong with the rational expectations revolution was that it was perfect information back by another description. It assumed that people were re-computing everything all the time. Once they are not, there is no reason to hold rational expectation. We could get rid of a whole load of micro once we became truth tellers.

That would also change our view of macro, because the view of macro that led us into the mess of 2008 was a view in which the economists had insisted it had to be build up from what the stupid representative agents were doing. If you returned to an earlier view of macro, not graduate school but undergraduate macro, most of what happened in 2008 and the extent to which we stopped the thing falling off a cliff in 2009 and 2010, through every Government pursuing expansionary fiscal policy just for a short period, is perfectly explicable by normal macro means.

Adair Turner

Can I say I completely agree with you? I said very carefully, you may not have noted, that some of micro had been helpful: the empirical parts of micro, the behavioural economics of micro and the bits of micro that explicitly rejected rational expectations and choice, for instance the behavioural stuff that feed into auto-enrolment systems. I completely agree with you that the fundamental problem of macro is that, about 40 years ago, we developed a hypothesis that we had to have micro foundations of macro theory. The micro foundations on which we built it were the most absurd you could ever imagine.

I think it reasonable to say that some of micro since then has moved away from those incredibly simplistic foundations, and macro has stuck in a rational expectations world that a lot of the best micro now rejects. That is what I would answer.

Participant

The gist of what both of you are saying is right, but there are some serious things wrong with it. Rational expectations, let us be clear, was a macro concept. It was an attempt to do exactly what Adair describes and, indeed, you described at the end, which was to build macroeconomics and micro foundations. The idea that rational choice is a micro foundation is just rubbish. It seems to me quite wrong. There are a lot of situations in which rational choices, at the micro level, have quite a lot of explanatory power. They do not always, but sometimes they do and sometimes they do not.

What was a mistake, as Adair was describing, was the 40-year attempt to build macro on micro foundations. That looked terribly sensible and plausible; it is just a project that, to my mind, basically failed and that is a large part of the source of the dissatisfaction with the performance of economics, which is around this table. Adair was right when we began in saying that the main problematic areas of economics today are in macro and in finance theory. He is right in saying he did not find economics very useful in one job and did in the others.

Participant

Could I just ask a quick question arising from that? Are you saying that the attempt to build macro on micro foundations was itself misguided or that the micro foundations were wrong?

Participant

I believe that the attempt was misguided in retrospect, because the simplifications you needed, in order to construct a macro story on micro foundations, were so extensive as to involve ruling out most of what really described the micro world.

Participant

Do you want to say something directly in reply to that?

Participant

There is a whole set of questions, something of which Stephen wrote about in methodological individualism a long time ago. He may come in and offer things about it. I think two things. One is that that mistake was, to my mind, extraordinarily analogous to what happened in biology. The publication by Dawkins of *The Selfish Gene* was exactly the same claim that what had to be happening at the genetic level must be explaining what is happening up here. Of course it is the case that, at any moment in time, whatever is occurring has some underpinnings. If I slide my hand across this table and say, 'The table is smooth,' but it would take me a mind-boggling amount of time to describe it in terms of the subatomic particles. On the other hand, there are bound to be subatomic particles for which that is true.

Whether it would be helpful to try to carry on the world in which we said, 'Hold on a minute, I just want to do the quantum physics in order to explain to you that the table is smooth,' the mistake that macro was making was thinking it would actually reach useful concepts down at that subatomic level, quickly and helpfully, and that there was no feedback mechanism. Biology tells you that there is strong feedback. It may even be a mistake to try to locate some things at the lower level.

Participant

As the sole sociologist in the room, which makes me a bit of a kicking boy today, I am becoming very frustrated by the turn that this conversation has taken from the excellent beginning that we were given. Actually, I would have had our speakers push a little harder. What we are now arguing about is what kind of economics best represents the world. That once again posits that there is a world of the social, if you like, unproblematically out there, away from economic theory.

What economics does not know that anyone in science and technology studies, sociology or whatever knows is that social scientific methods are irredeemably mixed up in the construction of the social. We cannot talk about the economy without economics, in the same way that we cannot talk about obesity without statistical measures of population health and so forth. That to me is the root of economics' power. It is completely complicit in the construction of the economic world in which we are all operating. The question should not be what kind of economics best represents the world that is out there but, when we represent the world out there with a particular kind of economics, what do we do? That is the crux of this debate and a really important account of how economics gains its power.

Thomas Palley

Nancy began by talking about Popper. I guess I have some reading, because I am still working with a barbarian model of sorts. That falsifiability is about as good as we can get. It is disturbing to hear that the experts now think that it does not work, but it did provide a very operational way of doing economics. What is startling and condemnatory about the way

economics has been done over the last 40 years is that even that way of doing it was completely disregarded.

The two examples that come to mind are very early on, around the 1980s, when people like Hugaer[?] were looking at real preference stuff. They contradicted the theory. Kahneman and Tversky's stuff has been around for 40 years' time and they contradicted it too. It was just completely disregarded. When the evidence, which is scarce, comes in a way that is condemnatory it is disregarded and it goes on. I remember going to meetings in the 1980s and hearing Robert Barro saying it is the only game in town. Of course it is not; there are lots of other games. This is the way we did it in macroeconomics. Sargent himself in the 1970s had the most fundamental theory about the natural rate of unemployment, and the implications of that or expectations tied with it were observationally equivalent, empirically, with the Keynesian model. It was completely disregarded. Evidence seems to have played no role in economics over the last 40 years. That is very important.

If I take you to the micro foundations part, I am troubled by the way that the discussion is that the project is wrong. I do not think the project is wrong. We do need some sort of micro foundation, if we are going to have some concept of the Keynesian. I believe in that. I think we are socially constructed agents. John likes to talk about you addressing the atomised agent and some sort of socially embedded agent, but that will be in micro foundations. What we come back to is the problem that economics has always insisted on neoclassical micro foundations. I am very micro founded also, but it is just not neoclassical micro foundations.

That is because there was a rejection of pluralism. The concept of pluralism becomes key. It is not pluralism because I like difference or I am a nice guy. I am pluralistic because your hypothesis, which I happen to disagree with, passes the tests that we have right now. The metaphor with which I always work when I think of economics as a science is that hypotheses have to pass through a grid. In physics, the natural sciences as we know them, the grid is very fine and so only a few hypotheses pass through this test. In economics, the grid is very coarse and Marxist theories can get through; Keynesian theories can get through; neoclassical theories can sort of get through too. Therefore, that forces us to live with pluralism and that is what we do not want. When we come back to the question of power again, pluralism is challenging to the powers that be.

Perhaps I should just throw out a caution against – in physics the grid is very fine – in economics the grid is very coarse – therefore that forces us to live with pluralism – because this is challenging to the powers that be. I caution against [inaudible] of what we can do. There are limits to what they can do and limits to what we can do as well.

Robert Skidelsky

We might come back to this later. I want the last word on this session. My project is not to build up macroeconomics from micro foundations, but to build micro foundations from macroeconomics. That was actually Keynes' project, but it did not succeed. That is what he was trying to do.

[Break]

John Kay[?]

I wanted to note that the power of rational expectations was that it was an answer to the performativity problem: that is to say, the agents knew what the model was. Actually, any other assumption is terribly difficult, which is why the microfoundations project almost had to be based on rational expectations, which in reality turned out to be inadequate.

Steven Lukes

If you are talking about biology and maybe chemistry, there is a third way. We do not just have to go from the top to the bottom or from the bottom to the top. In biology, a lot of what we are doing is just classification. It is very enlightening: we know what a mammal is, what constitutes a mammal and how they behave and what their characteristics are. We do not think about that in economics, but it could be extremely helpful. You could classify cases like [inaudible] Korea. That is empirical. You call it classification. You say, 'What makes a country that goes from below Congo to 20 times Congo? What is it about Korea and Taiwan? What classifies a country that can pull that off?' We do not need a big empirical model. We can just look at what they are doing. We are not going that in economics and we should be doing it.

Participant

That is politically very useful as well. I have often found [inaudible] talk about different types of capitalism, which fits in the taxonomy approach. Of course, part of what we have to do here is not just theory development or metatheory development but the political avenue as well. That is a way of getting these types of ideas into the public discourse.

Andrew Graham

I think we will move on. Can I just abuse my position as the Chair to reply briefly to Steven? I was not wanting to claim that you would never be seeking to seek to explain both ways around, bottom-up and top-down, but I think there are occasions when you ought to be asking, 'What is the issue you are trying to address? At what level does it arise?' There will be some questions that are almost necessarily system- or macro-level questions. You may be able to get some insight sometimes from the lower level, but you are more likely to be able to have the language to say something helpful by staying at that level. That is what my position is.

However, before we erode the time available to people this afternoon, Robert will be back in the Chair very shortly. Since he is out of the question, is this not a good moment to thank him for drawing together such an amazing set of people? I am now going to pass over to him the small topic of the Keynesian revolution and the theory of countervailing powers.

References

1. Atkinson, A. B. (1998). *Poverty in Europe*. Oxford: Basil Blackwell.
<http://eu.wiley.com/WileyCDA/WileyTitle/productCd-0631209093.html>
2. Deaton, A. (2001). Counting the world's poor. *World Bank Research Observer*, 16, 125–147.
3. MacKenzie, D., & Millo, Y. (2003, July). Constructing a market, performing theory: The historical sociology of a financial derivatives exchange. *American Journal of Sociology*, 109(1), 107–145, 122.
4. Popper, K. (2013). Science: Conjectures and refutations. In A. Bird & J. Ladyman (Eds.), *Arguing about Science* (p. 16). Abingdon, Oxon: Routledge.